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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impacts of aerosol particles on the microphysical and radiative properties of stratocumulus clouds over the Southeast Pacific ocean" *by* C. H. Twohy et al.

C. H. Twohy et al.

twohy@coas.oregonstate.edu

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Response to Reviewer 2

Major Points:

1) We are sorry that this reviewer didn't find our paper as interesting as the other reviewers did. However, we appreciate his/her comments and have used them to improve the flow of the paper. We understand that it may not have been clear to those not involved with VOCALS how all the diverse parts of the paper fit together. We have added text throughout to help alleviate this problem.

More details of many technical aspects of the paper, for example the WRF-Chem results, the LES modeling, and the electron microscopy, are given in other published or in-progress work. We collaborated with experts in these areas in order to add information specific and unique to our goal of interpreting aerosol-cloud interactions and their radiative impacts in the VOCALS region. We actually believe that this inclusion of diverse techniques is one of the strengths of our paper. To provide complete details on each of these supporting components, however, would have resulted in an overly long and overwrought paper, and we rely on references where needed.

2) Another major objection of this reviewer was that there was little new information in the paper. We began by independently looking at relationships between particle concentrations and droplet concentrations with distance from shore, similar to what others have done (Bretherton et al. ACP, 2010, Allen et al., ACP 2011). We then added additional analysis in order to distinguish this work from others and further the understanding of which and how particles influence clouds in the southeast Pacific. These related areas include the modeling of the fate of smelter-generated particles, measurements and modeling of which particle sizes form cloud droplets, and calculations and measurements of their ultimate impact on cloud albedo. In fact, these various lines of inquiry have led to several distinctly new conclusions, including:

a) That while continental aerosols are the primary ones causing smaller droplet sizes near shore along 20°S, these are not, primarily, smelter-generated particles. Prior to this work, smelters were assumed to be the primary source. b) That a wide variety of particle sizes are involved in stratocumulus cloud nucleation and not just the larger, lower Sc ones. This has broad implications for which pollutant particles, and at what stage in their lifecycle, may influence clouds and climate. c) That smaller effective radii near shore result from a combined influence of continental aerosol enhancement and physically thinner clouds. The net effect is that albedo is actually lower closer to shore than in unperturbed regions offshore.

These new results were originally in the Abstract and Conclusions, but we have re-

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worded slightly to make them more apparent.

3) More experimental detail and more diverse references were requested, and we agree that this was needed. We have added more information on how we defined flight legs and their averages, how we filled in missing data, and clearly defined "in-cloud" based on similar criteria to others (see below). We added a table (new Table 1) with all flight legs and times used. We added information on uncertainty and standard deviations of the measurements to the figure captions.

4) The reviewer thought we had a disproportionately high number of references from our own group. After responding to the other reviewers' comments, about 1/4 of the references were first-authored by people who also contributed to this paper. Some of these were necessary to document instrument techniques/performance and to explain various other aspects of the VOCALS experiment. However, we have substituted/supplemented with several other references when possible.

Minor Points:

1) Section 3.1: a) Given are basic results on patterns vs. distance from shore that feed into the later results. Differences from other work are the inclusion of the UHSAS concentrations and additional exploration of fits for particle concentration vs. droplet concentration, as well as assessment of statistical significance. b) We do not see that the 3rd and 4th paragraphs are expected results, as they specifically relate to the plots with real data shown in Fig. 1. c) The last paragraph ties our results into those processes described by others. While we could have put this in the Introduction, it seemed to fit better here. d) Transition to Section 3.2 has been added.

2) Section 3.2: a) Transition included to section 3.3. b) "Sea-salt" was standardized in our original manuscript, but one hyphen was omitted in copy-editing. c) 3.2.1: We chose these particular samples as representing the most polluted air near shore, with high particle concentrations. We have added results for some additional, less polluted samples in response to the reviewer's comment. We're also added sample sizes. See

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also 1st paragraph of response under “Major Points”. d. Agreed; this discussion was moved to Methods section and updated in light of the slightly higher mean droplet concentrations resulting from the new LWC criterion (described below) and the maximum droplet concentrations requested by Reviewer 3. e) 3.2.2: The Arizona smelter study is the only one known with similar data for comparison. This is all related to where the enhanced particles that change the cloud properties come from. The connection is now made in the text.

3) One comment by the reviewer led to a number of changes in the paper details. This was to make our definition of “in-cloud” more specific. We revised the in-cloud leg times in two ways. First, we used the exact same time periods as the mission manager’s defined start and end times, listing these in a new Table 1. Also, we eliminated cloud gaps more formally, using a liquid water content threshold > 0.05 g m-3, one that VOCALS researchers have used. As a result, all cloud-variable averages have been recalculated and all related plots re-rendered. This resulted in slight changes in calculated relationships summarized in Table 2 (formerly Table 1), but no significant change in the conclusions of the paper.

4) Title: We explored whether “ocean” should be capitalized in this context. The convention is unclear, as roughly half of prior VOCALS papers capitalize it in their titles and half do not. However, we agree with and acquiesce to the reviewer’s request.

5) Period added.

6) Fig. 5 caption: The colors portray the sulfur emissions of each smelter predicted from emissions inventories (assumed to be emitted as SO2) in Gg/yr. Clarified in caption.

7) Section 3.3.2: Part of what we tried to show is that each size distribution set is different, as the fraction of different particle sizes present is dependent on the thermodynamic history of the parcels collected. This is discussed and related to the rest of the paper.

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Corrected “complete”.

8) We feel that 3.4 is one of the most important sections of the paper. It completes the story of how increased aerosol number affects cloud microphysical properties and how they, together with cloud macrophysical properties, ultimately influence cloud optical depth and albedo in this region. We have adjusted this section slightly to make this more apparent.

Thanks to the reviewer for pointing out ways to unify diverse aspects of the paper.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 19715, 2012.